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Chapter 7 Friedman and the Walrasian Equations of The Natural-Rate Counter-Revolution

7.1. Introduction

From the 1930s, economic controversy has been a tale of three cities (Chicago and the two Cambridges) and three General Theories. In the 1930s, there were, in addition to the General Theory of Employment (Keynesian Macroeconomics), two other revolutionary attempts to don the mantle of generality: the General Theory of Method (the formalist revolution, involving structural econometrics and Walrasian general equilibrium) and the General Theory of Value (organised around the concept of monopolistic, or imperfect, competition). The Keynesian and formalist general revolutions became symbiotic and dominated the post-war landscape of economists. In contrast, the monopolistic competition revolution did not readily lend itself to general equilibrium formalism and, so far, has yet to achieve its promise (Tinbergen 1967, 268).

Edward Chamberlin (1957, 296) described the focus of opposition to the last of these three General Theories as "The Chicago School of Anti-Monopolistic Competition"; only then did economists begin to refer to Chicago as a School (Stigler 1988, 150).ⁱ Later, the term "monetarism" was coined to describe the Chicago opposition to the Keynesian General Theory of Employment. For Chicago economists, the 1930s exhibited "an excess of originality" (Stigler 1955b, 301). The purpose of this essay is to discuss Milton Friedman's opposition to the Walrasian component of the General Theory of Method.

Two of these revolutionary research agendas (Macroeconomics and Method) acquired post-war hegemonic ascendancy. But the two most influential revolutionary economists of the twentieth century were more united in their *opposition* to the General Theory of Method than is commonly supposed. J.M. Keynes and Milton Friedman had similar - and sceptical - views about econometrics. Keynes also informed Hicks that "Walras' theory and all others along those lines are little better than nonsense" (cited by Skidelsky 1992, 615). Keynes (1936a, 177) contrasted his own *General Theory* with a 'classical' caricature; Walras, he believed, was strictly in this classical tradition.

In apparent contrast, Friedman (1968a, 8) constructed his anti-Keynesian counter-revolution using Walrasian language: "At any moment in time there is some level of employment which

has the property that it is consistent with equilibrium in the structure of *real* wage rates ... The 'natural rate of unemployment', in other words, is the level that would be ground out by the Walrasian system of general equilibrium equations, provided that there is embedded in them the actual structural characteristics of the labor and commodity markets [emphasis in text]". Later, he augmented the quantity theory with the Walrasian equations of general equilibrium (1974b, 31-2). According to one of his most severe critics, "Friedman, like all mainstream theorists, accepts the Walrasian system as the microfoundations of macroeconomic theory" (Davidson 1989, 9).

But Friedman's research has always been in the Marshallian methodological tradition (Hammond 1996) and Alfred Marshall was regarded as the "patron saint of 'positive economics'" (Clower 1964, 367). Robert Clower (1965) and Axel Leijonhuvud (1967)ⁱⁱ had recently questioned the legitimacy of the Walrasian Keynesianism of the Neoclassical Synthesis, and Clower (1964, 372) concluded that the Friedman and Schwartz research project was an assault on this neo-Walrasian orthodoxy: their conclusions were "bound to be a bit upsetting to those vision of the working of the economic system is informed by neo-Walrasian theoretical conceptions, which is to say all but a small handful of contemporary economists". Friedman (1974b, 159-60) was aware of this Walrasian dimension of the struggle for influence: "Tobin's style goes further in Walras's direction than mine does ... this difference in methodological style is an important reason why we seem to talk at cross purposes ... Patinkin, even more than Tobin, is Walrasian, concerned with abstract completeness, rather than Marshallian, concerned with the construction of special tools for special problems". Patinkin and Tobin (Friedman's Walrasian critics) objected to the *policy conclusions* of the (Walrasian) natural-rate model. The purpose of this chapter is to place Friedman's use of these equations in the context of his other statements about the limited role that should be allocated to Walrasian-style thinking.

Friedman's words were some of the most influential words ever spoken by a President of the American Economic Association (AEA); they launched the ongoing natural-rate research project around which modern macroeconomics has been organised for the last three decades. The natural-rate of unemployment is typically presented as a hard empirical constant, or as an empirically valid variable that changes only slowly. It is a relatively unobjectionable concept, in so far as it represents a speed limit, beyond which inflation will increase, and beyond which the associated gains with respect to unemployment will be temporary. But it also supposedly represents a gravitational force which ensures that *disinflation* will have only temporary consequences: unemployment will, in time, return to its natural level (thought to be about 2% in Britain in the mid-1970s, when the concept began to acquire overriding policy influence). The apparent paradox discussed in this chapter is that Friedman was - and continues to be - highly

sceptical of such empirical measures. He also described as "utterly unattainable" the accurate measurement of inflationary expectations (the equilibrating variable of the natural-rate model).

Friedman's (1968a, 14-15) AEA Presidential Address was a critique of "*Employment as a criteria of policy*" [emphasis in text]. The apparent purpose of his counter-revolution was apparently not to launch a natural-rate estimating industry, but to suggest that using monetary policy to target unemployment was "like a space vehicle that has taken a fix on the wrong star. No matter how sensitive and sophisticated its guiding apparatus, the space vehicle will go astray". Thus, currently fashionable monetary policy rules (which suggest that interest rates should be fine-tuned to counteract deviations of current output or unemployment from numerically calculable natural levels) represent a reversal of Friedman's counter-revolution. They also represent (in Friedman's terms) the use of an abstract Walrasian concept in a practical area where only Marshallian tools are relevant.

To avoid ambiguity in the use of the terms 'Marshallian' and 'Walrasian', this chapter follows Friedman's (1974b, 143, 146, 159) use of these terms: the Walrasian approach is "concerned with abstract completeness", in contrast to the Marshallian approach, which is "concerned with the construction of special tools for special problems". Section 7.2 analyses Friedman's views on the Walrasian system. Friedman argued that it was unfortunate that Walrasian economics had overtaken Marshallian analysis. Formalism, Friedman argued, yielded few conclusions that were susceptible to empirical contradiction, and tended to rely on assertions about inflationary expectations that were empirically "utterly unattainable" to measure. Walras' "divorce of form from substance had led to some "nonsense".

Section 7.3 places the Walrasian equations of the natural-rate counter-revolution in the context of Friedman's analysis of the limitations of Walrasian analysis. The implication of Friedman's analysis is that the vertical long-run Phillips curve is a "language proposition" while the short-run Phillips curve is a "substantive" proposition. The important question is empirical: some estimate can be made of a rate of unemployment to which the title "natural" can be attached; but does this supposedly natural-rate exert any influence on the course of the actual rate? But this crucial empirical question is rarely addressed by those who estimate natural-rates. Friedman also stated that unemployment was a "highly inefficient method" of adjustment - although increasing unemployment (to reduce inflationary expectations and shift the short-run Phillips curve downwards) is the adjustment mechanism of natural-rate models. Section 7.4 provides a brief outline of the process by which the natural-rate became influential in macroeconomics. Concluding remarks are provided in section 7.5.

7.2. Friedman on Walrasian Economics

Friedman (1996a, 1989) describes himself as "a long term Marshallian"; the label he put on his methodology is "Marshallianism" (Hammond 1996, 30). One of Friedman's (1940; 1941) earliest contributions to economic disputation was a critical review of Jan Tinbergen's macroeconometric project; this was followed almost immediately by a review of Robert Triffin's *Monopolistic Competition and General Equilibrium Theory*. Triffin (1941, 3) argued that the "gravitational centre" of Marshallian economics was the industry: "What we might well now do is to restate the whole problem in terms of the Walrasian, general equilibrium system of economic theory". Friedman (1941, 390) replied that "For these problems, we must continue to employ the Marshallian tools, until better ones are invented".

Paul Samuelson (1983, 7) recalled that Frank Knight (the doyen of inter-war Chicago) was fond of exclaiming that "If there is anything I can't stand it's a Keynesian and a believer of monopolistic competition". Friedman (the doyen of post-war Chicago) made his earliest contributions to the Chicago cause in opposition to two of the General Theories spawned by the 1930s (the Walrasian approach and monopolistic competition). As he explained to his students in the late 1940s, Marshall's *Principles* was "still the best book available in economic theory. This is indeed a sad commentary on the economics of our time. Marshall's superiority is explained by his approach to economics as contrasted with the modern approach" (cited by Hammond 1996, 31). Yet, it was Friedman's AEA Presidential use of Walrasian language which "*undermined ... the whole intellectual basis of post war demand management by government*" [emphasis in text] (Laidler 1975, 45). A Marshallian persuaded the economics profession that the "gravitational centre" of the macroeconomy was the Walrasian natural-rate of unemployment.

Friedman (1953, 89-93) noted that "by slow and gradual steps, the role assigned to economic theory has altered in the course of time until today we assign a substantially different role to theory than Marshall did. We curtsy to Marshall, but we walk with Walras". According to Friedman, the important distinction between "the conceptions of economic theory implicit in Marshall and Walras lies in the purpose for which the theory is constructed and used". For Marshall, economic theory was "an engine for the discovery of concrete truth". In contrast, "Abstractness, generality, and mathematical elegance have in some measure become ends in themselves, criteria by which to judge economic theory ... much recent work on Keynes's theory of employment is Walrasian ... so is current economic theory in general". The fundamental distinction between Marshallian and Walrasian economics "is treating economics as a serious subject versus treating it as a branch of mathematics, and treating it as a scientific subject as opposed to an aesthetic subject" (Friedman, conversation with Hammond 1990, 168).

Much of the Walrasian formalist work took place at the Cowles Commission, during its sojourn at the University of Chicago. The Walras centennial program in Chicago, hosted by the AEA, the Econometric Society and the American Statistical Association, stimulated a wide revival of interest in Walras; from the 1930s, general equilibrium was "in the air" (Jaffe 1935; Menger 1973, 50-1, 57 n24; Weintraub 1983, 17, 19, 37). Between 1946-48, Friedman was a frequent participant at Cowles Commission seminars. His relentless criticism of their econometric projects prompted Tjalling Koopmans to retort: "But what if the investigator is honest?" (cited by Epstein 1987, 107). Koopmans was reported to be relieved when he and the Cowles Commission left the University of Chicago, because his students and colleagues (such as Harry Markowitz and Gerard Debreu) had their work criticised as being mathematics rather than economics. According to Beckman (1991, 264-5, 253) the source of this antagonism was a Chicago economist whose "star was just rising" and who later won a Nobel Prize. His identity can be determined by reference to the period (1944-55) Koopmans spent at Chicago.ⁱⁱⁱ Certainly, Markowitz (1992, 286) concluded his Nobel Lecture with the recollection that Friedman had attempted to persuade his dissertation committee not to award his PhD on the grounds that portfolio theory was not a legitimate part of economics.

Oscar Lange's 1944 Cowles monograph *Price Flexibility and Employment* challenged the Chicago view of general equilibrium theory (Reder 1982, 5). Immediately, Friedman (1953 [1946], 277-300) led the "Methodological Criticism" on Lange's "shackles of formalism ... the analysis seems unreal and artificial ... more nearly a rationalisation of policy conclusions previously reached than a basis for them ... not a shred of evidence is offered for them". Friedman criticised Lange's "use of classifications that have no direct empirical counter-part ... The resulting system of formal models has no solid basis in observed facts and yields few if any conclusions susceptible of empirical contradiction". Friedman's reaction to Lange is interesting for its discussion of the complications associated with monetary changes, and the impossibility of incorporating an empirical counter-part to inflationary expectations: "An example of a classification that has no direct empirical counterpart is Lange's classification of monetary changes ... An explicit monetary policy aimed at achieving a neutral (or positive or negative) monetary effect would be exceedingly complicated, would involve action especially adapted to the particular disequilibrium to be corrected, and would involve knowledge about price expectations, that even in principle, let alone in practice, would be utterly unattainable".^{iv}

In Chicago in the 1950s, Friedman was "excessively negative" about the "sterile" and "untestable" nature of general equilibrium analysis (Becker 1991, 143). But the year after Friedman's methodological essay, Kenneth Arrow and Gerard Debreu (1954) demonstrated the

existence of a general equilibrium solution (with perfect competition and forward markets in all goods and services); Walras increasingly came to be seen as the forefather of modern microeconomics (Debreu 1984, 268; Schumpeter 1954, 827).^v William Jaffe's (1954) translation of Walras' *Elements of Pure Economics* was published for the AEA and the Royal Economic Society; and Friedman (1955), as one of the leading methodologists of his era, wrote a critique of 'Leon Walras and his Economic System' for the *American Economic Review*.

Friedman (1955a, 906-7) and Stigler (1949b, 38) noted that Marshall was Second Wrangler in mathematics, and that Walras, in contrast, had twice failed the entry examinations for the Ecole Polytechnique. Friedman (1955a, 904-9) argued that using "very elementary mathematics indeed", Walras' work has led to a "misconception" of economic theory. His general equilibrium system possessed "an extraordinary aesthetic appeal as a beautifully articulated abstraction", but the failure to distinguish between the "task Cournot outlined and the task accomplished by Walras ... seems to me to be a primary source of methodological confusion in economics ... [Walras'] problem is the problem of form not of content: of displaying an idealised picture of the economic system, not of constructing an engine for analysing concrete problems ... [Cournot's] goal was an analysis that would, given the relevant statistical material, yield specific answers to specific empirical questions ... ". Walras' "divorce of form from substance" had led to some "nonsense". The marginalist revolution assigned to *rarete* (marginal utility) "an almost metaphysical role ... 'it has no direct or measurable relation to space or time' [Walras, p.117] ... He says nothing more on the subject and simply proceeds to take for granted that there is something called *rarete* which has numerical values that can be plotted ... emphasis on pure form has an important role to play in providing a language, a classification scheme to use in organising materials - labels, as it were, for the compartments of our analytical filing box. This is Walras' great contribution".

One of Friedman's contributions has been to provide a classification scheme for all conceivable inflation-unemployment observations. Those who have followed him have 'taken for granted that there is something called the natural-rate of unemployment which has a numerical value that can be plotted'. Friedman's essay was written after a sabbatical at Cambridge where, in some powerful quarters, utility was regarded as a "metaphysical concept of impregnable circularity", and where Friedman's methodology may have had an influence: "The hallmark of a metaphysical proposition is that it is not capable of being tested" (Joan Robinson 1962, 48, 8). The (rarely undertaken) test of the natural-rate model concerns its ability to *attract* the actual rate.

Prior to *Studies in the Quantity Theory of Money* (1956), Friedman's (1953, 7) major influence

was as a methodologist: "viewed as a language, theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system for organising empirical material and facilitating our understanding of it". It was "factual evidence alone" which "can show whether the categories of the 'analytical filing system' have a meaningful empirical counterpart ... the relevant question to be asked is usefulness and not rightness or wrongness". Theory was perceived by Friedman (1976, 8) to be a series of substantial empirical propositions capable of being predictively tested: "The definition of a *demand curve* is 'theory as language'. However, the statement that the demand curve slopes downward to the right is theory as a substantive empirical proposition. It has empirically observable consequences, whereas the definition of a *demand curve* does not. Theory as language coincides with Marshall's *engine of analysis*. The objective is to construct a language that will be most fruitful in both clarifying thought and facilitating the discovery of substantive propositions" [emphases in text].

These demand curves are derived from a concept (utility) which may need no cardinal measure to assist the analysis. The value of the concept of the demand curve lies in its ability to organise "knowledge and thinking about a problem" and to provide qualitative and "quantitative estimates of the effects of various changes" (Friedman 1976, 34). Friedman's framework suggests that the long-run Phillips curve is a language proposition, whereas the shape and gravitational characteristics of the short-run Phillips curves are substantial empirical propositions. In the disinflation zone, the natural-rate model adds value by providing quantitative estimates of the magnitude and duration of the unemployment required to reduce inflation to an acceptable level. But it is these substantial empirical propositions which are frequently less-than-adequately analysed by those who present estimates of the natural-rate of unemployment.

7.3. The Walrasian Equations of the Natural-Rate Counter Revolution

Keynes (1943, 185; JMK XIII [1932], 406; [1934], 486-7) noted that "the weapon of deliberately creating unemployment ... to confine the tendency of wages to rise beyond the limits set by the volume of money ... [is a] weapon the world after a good try, has decided to discard". He constructed his policy revolution against the "orthodox equilibrium theory" which saw strong "natural forces" bringing output back to its optimal level.

But the Walrasian natural-rate model became the Marshallian "special tool for the special problem" of formulating an appropriate policy response to the high inflation of the 1970s. The model assumes (usually without any supporting evidence) that there exists strong "natural forces" pulling output and unemployment back to their natural levels. The natural-rate of unemployment is an *abstract* long-run concept; but the path towards it (if it exists and if it

provides a magnetised trail for the actual rate of unemployment) is dependent upon the *actual* short-run characteristics of the economy in response to 'unnatural' levels of unemployment. Friedman (1974b, 150) specified that "The long-run equilibrium in which, as I put it, 'all anticipations are realised' and that is determined by 'the earlier quantity theory plus the Walrasian equations of general equilibrium' is not a state that is assumed ever to be attained in practice. It is a logical construct that defines the norm or trend from which the actual world is always deviating but to which it is returning or about which it tends to fluctuate." The correctness of the hypothesis "is a question of fact to be determined by the consistency of the hypothesis with experience".

The natural-rate model is a hypothesis to be tested (if it is capable of being falsified); it is not a species of revealed truth. As noted above, Koopmans referred to the "Friedman critique" of econometrics; Don Patinkin (another Cowles economist) described the "Friedman question" as "under what circumstances would you abandon your pet theory?" (cited by Leeson 1998, 443-4). Friedman (1974b, 1) claimed that the quantity theory framework "has probably been 'tested' with quantitative data more extensively than any other set of propositions in formal economics - unless it be the negatively sloped demand curve". The negatively sloped demand curve coincides "with Marshall's *engine of analysis*"; but Friedman's (1968a, 9) quantity theory contains the proposition that the "natural" rate of unemployment analytically separates "real forces from monetary forces". Estimates of the Walrasian natural-rate of unemployment which emerge from these "real forces" are rarely subjected to "Friedman's question".

"At any moment in time", *if* the grinding of the Walrasian equations were possible, then a natural-rate of unemployment might emerge from those structural equations. But the implication of Friedman's view is that the crucial question is empirical: is the actual rate of unemployment gravitating towards or fluctuating around some estimate of equilibrium unemployment? Before the natural-rate concept is invested with any validity it must first pass the empirical test: is the actual rate returning to the natural-rate? There was no evidence to suggest that there were strong gravitational forces at work in the British economy which were returning the actual rate to the natural rate. The British evidence suggests that the natural rate is an untestable and unfalsifiable concept - an estimate of some abstract measure of unemployment that is graced with the unjustified title of 'natural'.

The natural-rate model *implies* (usually without any supporting evidence) that it is possible to provide policy-makers with accurate econometric estimates of the magnitude of the natural-rate of unemployment, and that this natural-rate exerts a reliably strong gravitational pull on the actual rate. Measured unemployment (U) differs from its natural level (U^N), only because of

expectationary disequilibrium (i.e. inflationary expectations, ΔP^e , are not equal to actual inflation ΔP). Thus, any 'unnatural' (U^{UN}) divergence of U from U^N is a function of the speed of adjustment (α) of incorrect inflationary expectations.

The natural-rate model can be expressed as:

$$U = U^N + U^{UN} \quad (1)$$

$$U^{UN} = f [\alpha (\Delta P^e - \Delta P)] \quad (2)$$

While U^N can be reduced by microeconomic manipulation (improving labour market flexibility etc), macroeconomic policy can effect disinflation only by increasing U above U^N ; the speed of reduction of ΔP and therefore U^{UN} depends on α - the delusion variable. 'Unnatural' rates of unemployment is therefore attributed to this 'delusion' and will reduce to zero as inflationary expectations cease to be inaccurate. Equally, macroeconomic policy can not sustainably reduce U below U^N , without incurring the cost of accelerating inflation. But at the core of this model lie two variables (P^e and U^N) which, Friedman has argued, are either impossible or extremely difficult to accurately measure.

Friedman's framework implies that the vertical long-run Phillips curve is a language proposition; the shape of the short-run Phillips curve, and the gravitational pull of the natural-rate (and hence the speed of adjustment) are substantive propositions. The shape of the short-run Phillips curve in the natural-rate model (the crucial mechanism for the disinflation adjustment mechanism) is noticeably different from the shape of Phillips' (1958) and Lipsey's (1960) curves as unemployment reaches four or five per cent. The data (in contrast to the natural-rate model) suggests the existence of an important degree of downwards wage inflexibility - there appears to be an expectations trap preventing inflationary expectations from falling. As a substantive empirical proposition, the natural-rate of unemployment appears to be model specific, and not a general property of the macroeconomy.

Friedman (1953, 165) cautioned that "Wage rates tend to be among the less flexible prices", and thus unemployment was "a highly inefficient method" of adjustment, because the "adjustment will not have been completed until the deflation has run its sorry course".^{vi} Later, Friedman (1977, 454; 1976, 215) thought that he saw in Phillips' work evidence of "deflation" and "falling wages" at higher levels of unemployment. Phillips (1958, 283), in contrast, found that in his "highly non-linear" relationship, "wage rates fall only very slowly". In Phillips' data there were eight examples in the post-1904 period of falling wages (with unemployment ranging from ten to twenty-two per cent); high levels of unemployment were more commonly associated with

positive rates of wage inflation. With this degree of downwards wage stickiness, the natural-rate model suggests that 'unnatural' levels of unemployment would persist for lengthy periods.

7.4. The Walrasian Colonisation of the Profession

Although Keynes was sceptical about the Walrasian approach, Hicks' *Value and Capital* (1939) was self-consciously in the Walrasian tradition, as was Samuelson's *Foundations of Economic Analysis* (1947).^{vii} These two books, together with *The General Theory*, were the foundations of professional training in the post-war period, and Keynesian macroeconomics came to be perceived as "a short cut 'general equilibrium' theory". Since then, Walras and Marshall "have been contesting for the souls of economists" (Tobin 1987, 118; 1972, 104-5; Hicks 1934, 347; Dreze 1991, 7-8).

In his AEA Presidential Address, Friedman (1968a, 10) concluded that the monetary authorities "cannot know what the 'natural' rate is. Unfortunately, we have as yet devised no method to estimate accurately and readily the natural rate of either interest or employment". Three decades later, this empirical measurement exercise was still out of reach: "As the coiner of the term, I am disturbed at its widespread misuse and misunderstanding. The natural rate is not a fixed number. It is not 6% or 5%, or some other magic number ... The natural rate is a concept that does have an empirical counterpart - but that counterpart is not easy to measure and will depend on particular circumstances of time and place" (Friedman 1996b). But Friedman's Address was followed by numerous attempts to quantify this supposedly natural rate of unemployment. Social Science Research Council funding for the Manchester Inflation Workshop began in July 1971; David Laidler (1975, 45), in presenting the "implications of [Friedman's] ideas for our understanding of the British economy", reported that the "preliminary results of work in progress at Manchester University" suggested that the natural-rate of unemployment was "perhaps a little less than 2% in Britain, although such an estimate is necessarily subject to a wide margin of error ... we shall nevertheless probably see an average of a million unemployed for five years or more if we are to get the inflation rate down below, say, five per cent by 1980". Laidler's judgement was that this was "too much unemployment for too long" and he argued that widespread indexation might reduce the unemployment cost of disinflation.

Laidler (1976, 71) concluded that "we therefore have no way of putting the expectations augmented Phillips curve to the test in a way which will generate results that command widespread assent; although he hoped that reliable price expectations data might subsequently be generated from survey data. Laidler (1975, 42) also discussed the possibility that the natural-rate "hypothesis" might be false. But in his Nobel Lecture, Friedman (1977, 459) declared that "The natural-rate hypothesis is by now widely accepted by economists"; the economy would

return, after disinflation, to the natural-rate. The policy choice was therefore a question of timing: "when reporters and others ask how much unemployment it would cost to reduce unemployment, I say to them, when did you last beat your wife? How much unemployment will it cost *not* to beat inflation? ... *if you continue to let inflation accelerate you are going to have higher unemployment either way*. So you only have a choice between which way you want the unemployment to come. Do you want it to come while you are getting sicker or do you want it to come while you are getting better?" [emphases in text] (Friedman 1975b, 32).

But this 1975 account is not a completely accurate representation of the vertical long-run Phillips curve model (neither does it fully describe the positively sloped long-run Phillips curve that Friedman later described in his Nobel Lecture). The higher unemployment that follows a policy-induced increase in inflation is the product of centripetal force: the benign 'return' to the natural-rate (which acts as a gravitational brake, halting the rise in unemployment beyond the natural-rate). But the higher unemployment that follows from policy-induced *disinflation* is the result of centrifugal force: unemployment increases beyond the natural-rate until the centripetal force of error correction (with respect to incorrect inflationary expectations) pulls the system back to centre at the natural-rate of unemployment. The first scenario is a constrained rise in unemployment; the natural-rate model tells us nothing, *ex ante*, about the level and duration of unemployment associated with the second scenario.

Ex post, the margin of error was revealed to be much wider than expected: "none of us expected the deep and prolonged depression that ensued ... the experience has been chastening (Laidler 1985). Patrick Minford (1994, 230) recalled that: "At the beginning of the 1980s, I was helping to push the incoming Tories towards the idea of a medium term financial strategy to control inflation and I tended to think of the natural rate of unemployment as something that would not be too outrageous a number. I don't think it ever crossed my mind that it was anything like three million". Measured unemployment increased from 2.1% in 1973 to 13% in 1985 (3.2 million) and remained over 2 million until January 1985 (Kavanagh 1990, 231-2).

It is often said that economists, like photographers, fall in love with their models; certainly Minford appears to be prepared to acknowledge a personal forecasting failure in preference to the idea that he was led into error by the natural-rate framework. According to Friedman's (1968a, 9) exposition, the natural-rate of unemployment should have *fallen* as a result of the labour market reform and diminution of trade union power of the Thatcher years. Any framework that can lead such economist to "go astray" by "taking a fix on the wrong model" must be regarded as suspect: either the natural rate inexplicably increased over six-fold in less than a decade, or the model is an unreliable guide to policy.

There is another alternative. The gravitational pull of the natural-rate may be so weak that "full adjustment to the new rate of inflation takes about as long for employment as for interest rates, say, a couple of decades" - which was exactly Friedman's (1968a, 11) AEA Presidential prediction. But in contrast to this pessimistic scenario, Patrick Minford informed the 1980 House of Commons Select Committee that (on New Classical assumptions), "the disturbance to output and employment from reduction in the money supply and P[ublic] S[ector]B[orrowing]R[equirement] would be minimal" (cited by Jay 1986, 208). Likewise, before the same Committee, Friedman (1980, 61, 56) predicted that from "the best evidence ... (a) only a modest reduction in output and employment will be a side effect of reducing inflation to single figures by 1982 [... a temporary retardation in economic growth] and (b) the effect on investment and the potential for future growth will be highly favourable". Unemployment was "an unfortunate side effect of reducing inflation"; only rigidities stood in the way of a rapid return to the natural rate of unemployment: "The mechanism causing the contraction in output is the slowing of nominal spending in response to the slowing of monetary growth and the inevitable lags in the absorption of slower spending by wages and prices".

Nearly all the discussion of the natural-rate model provided by Friedman relates to the behaviour of the economy on the expansionary side of the natural-rate, where increases in unemployment are constrained by powerful centripetal forces. The closest reference to policy-induced *disinflation* in his Presidential Address (1968a, 10) is the reference to monetary authorities choosing a target rate of unemployment above the natural-rate: "they will be led to produce a deflation and an accelerating deflation at that". In 'Wage Determination and Unemployment' there are ten examples of unanticipated changes in aggregate demand; but the first nine all relate to unanticipated *increases* in nominal aggregate demand (1976, 216, 222, 224, 226, 227, 230, 232, 233, 234). Friedman concluded that the natural-rate model was validated by experience: any resemblance between the model and "what has been happening in Britain is not coincidental: what British governments have tried to do is to keep unemployment below the natural rate, and to do so they have had to accelerate inflation - from 3.9 percent in 1964 to 16 percent in 1974".

Few economists would object to this explanation; Phillips (1962, 1-2), for example, noted that unacceptably high British inflation had been caused by the government maintaining employment at an "extremely high level". But this reveals nothing about the existence (or non-existence) of a natural-rate of unemployment. Neither does it provide any information about the behaviour of an economy undergoing *disinflation*. Of Friedman's ten examples, only the last discusses the main monetarist policy proposition, an unanticipated *decline* in aggregate demand:

"Conversely let there be an unanticipated decline in aggregate demand, so that employers are willing to hire fewer workers at each real wage rate as perceived by them. Workers searching for jobs will find fewer offers that, on the basis of their unchanged anticipations, are attractive enough to compensate them for giving up the search. The average time between jobs will lengthen, and so will recorded unemployment. As the less attractive employment situation becomes more widely known, job-seekers will revise their anticipation about opportunities, become less choosy, and recorded unemployment will decline towards its natural level" (1976, 235).

It is not clear why unemployed job-seekers should take two decades to become "less choosy", but this is the substantial empirical proposition of the natural-rate model. Perhaps this individualistic explanation of the cause of unemployment appealed to Mrs. Thatcher (1995, 126, 95, 417) who echoed both the sentiments and the language of Friedman's (1968a, 14-15) Presidential analogy of steering by the stars: "Alan [Walters]' view was that ... the monetary base was the best, indeed the only reliable star to steer by ... True, inflation had moved up from the low point it had reached after the [1983] election, and unemployment, always a lagging indicator remained stubbornly high ... [but we] knew how to control the money supply through interest rates and did so".

During the course of the 1979-83 Parliament, unemployment rose from 5.4% to 12.7% and industrial output fell by over 11% (Kavanagh 1990, 231). In 1980, Nigel Lawson informed the press that the "medium-term financial strategy is essentially a monetary - or if you like monetarist - strategy" (cited by Congdon 1989, 231). But when asked in January 1985 by Peter Jay: "with all-time record unemployment figures this week, have [we] yet reached that natural rate", Mrs Thatcher replied "It's not a doctrine to which I've subscribed. It's one which I think came in with Milton Friedman. I used to look at it, I used to look at it and not adopt it" (cited by Smith 1987, 122). Shortly afterwards, the *Financial Times* ran a lead article under the headline 'Monetarism Dead - Official'. But some economists continue to estimate the natural-rate of unemployment that supposedly results from unemployed workers losing their delusions and choosiness. Friedman (1976, 221) described as "somewhat ludicrous the confident statements that many economists had made about 'trade offs' based on empirically fitted Phillips curves". A similar (or harsher) judgement could be made about statements concerning the unemployment costs of disinflation based on the natural-rate model.

The leading Keynesian-formalist described the "virus" quality of Keynes' *General Theory* (Samuelson 1964, 315); but the 1976 edition of his textbook accorded only a footnote to the natural-rate model (Samuelson 1976b, 835, n8). Shortly afterwards, however, that model conquered the profession, in part, for reasons that Friedman found less than satisfactory about

Walras: it was "an elegant and concise representation of the inflationary process for the long-run" (Taylor 1979, 108; Blinder 1979, 19-20). Robert Gordon (1978), and Rudiger Dornbusch and Stanley Fischer (1978) incorporated the natural-rate model as the organising concept of macroeconomics into the first editions of their intermediate textbooks. But the relevant substantive empirical questions are rarely asked by those who present estimates of supposedly natural rates of unemployment. The various editions of Gordon's textbook, for example, present scientific estimates of the natural rate of unemployment in the United States from 1890 to the present day, with no examination of the "substantive" empirical question (including "Friedman's question").

Keynes had somewhat of a Cassandra complex and during the Monetarist decade many Keynesians shared this fate. Followers of the formalist and Keynesian revolutions displayed little immunity as the Old Keynesian era ended. For Keynes, the long run was a "subject for undergraduates" (Joan Robinson 1962, 75; Eshag 1963, 100, n118); and Robert Solow (1987, 183) complained that the way macroeconomists used the natural-rate of unemployment was an "intellectual scandal". But during the current period of Keynesian revival, the procedure of comparing "magic" estimates of the natural-rate of unemployment with the actual rate of unemployment to describe a monetary policy rule still retains an "amaz[ing] ... status" (Rogerson 1996, 86). Ironically, these anti-formalist objections echo Keynes' (1939, 559) complaint about Tinbergen: "The worst of him is that he is much more interested in getting on with the job than in deciding whether the job is worth getting on with".

There are no truly general theories in science; only competing explanations which, for a variety of reasons (not all to do with the 'classical' process) command varying degrees of respect among practitioners. The natural-rate model challenged its primary adversary, the high inflation trade-off interpretation of the Phillips curve, and is now challenged by models which invoke hysteresis, implicit contracts, insiders and outsiders, an expectations trap, efficiency wages etc. Not all of these models deny that "at any point in time" (to use Friedman's phrase) a natural-rate of unemployment might emerge from the Walrasian equations; but they tend to deny that the gravitational pull of any particular natural-rate is stronger than the gravitational pull of the *actual* rate of unemployment (Phelps 1996).^{viii} The positive co-movements of inflation-unemployment observations in the 1960s *appeared* to be a vindication of the power of the equilibrating forces of the natural-rate model; and this led to a widely-held conviction that these equilibrating forces could be relied upon (in the disinflation zone) as unemployment increased in the 1970s and 1980s. But the forces set up by both inflation and policy-induced recession seem to resemble unpredictable chain reactions rather than the attractive equilibrating forces of the natural-rate model - causing some Monetarists to question the validity of their earlier policy

optimism.

For Alfred Marshall (1920, 564) "The most valuable of all capital is that invested in human beings"; and increasing unemployment above the natural-rate tends to reduce the stock of human capital (thus increasing the natural-rate), leaving a large pool of outsiders who have only a limited ability to affect the wages of insiders. Thus the idea of a unique and stable equilibrium configuration exerting an all-powerful influence on the actual course of unemployment has been challenged by the idea that the natural-rate limps behind, and tracks, the actual rate, with (in Keynes' phrase) "not so lame a foot" (JMK XXII [1940], 120-1).

7.5. Concluding Remarks

In Stigler's (1983c, 210) judgement, had Friedman been a Walrasian, not a Marshallian, much of the Chicago research program would, he judged, have been thwarted.^{ix} But as Harry Johnson (1971) pointed out, the natural-rate model is silent about the short run (the speed and effectiveness of unemployment-induced disinflation): "The most serious defects of the Monetarist counter-revolution from the academic point of view are, on the one hand, the abnegation of the restated quantity theory of money from the responsibility of providing a theory of the determination of prices and output [analysing the supply response of the economy to monetary impulses ... whether monetary changes affected prices or quantities] and on the other hand, its continuing reliance on the methodology of positive economics ... Personally, I expect [Monetarism] to peter out".

After Friedman's AEA Address, Johnson (a Chicago colleague) and Patinkin (an ex-Chicago colleague) became two of Friedman's most bitter adversaries. Patinkin (1969, 1974) focused on the supposedly bogus role of an inter-war Chicago oral quantity theory tradition, while Johnson (1970, 85-86, 107, 48) suggested that Friedman had constructed his counter-revolution by imitating the tactics of the Keynesian revolution: "My personal hypothesis is that, as a result of his studies of the Marshallian demand curve and his year as a visitor at Cambridge, Friedman became enamoured of the 'Cambridge oral tradition' as a concept permitting the attribution to an institution of a wisdom exceeding that displayed in its published works, and unconsciously stole a leaf from Cambridge's book for the benefit of his own institution". With respect to Friedman, Johnson (1971) concluded that "one should not be too fastidious in condemnation of the techniques of scholarly chicanery to promote a revolution or counter-revolution in economic theory".

The truth-content of Friedman's 'oral tradition' continues to generate passionate scholarly interest; often involving speculation about what Johnson described as the "motivational

construction" behind Friedman's monetarist counter-revolution (Parkin 1986; Patinkin 1986; Steindl 1990; Tavlas 1998a and b; Laidler 1993, 1998a and b; Leeson 1998). It certainly appears that Friedman and Stigler brought a considerable degree of sociological perceptiveness to their assault on (and defence of) various aspects of economic orthodoxy. Friedman enhanced his policy-revolution by embracing the *language* of his opponents (IS-LM, econometrics, income-expenditure, money demand); a language that he was often sceptical about. As a *language revolution*, the natural-rate model is comparable, in terms of influence, to the Keynesian revolution that it sought to overthrow.

Friedman's macroeconomics was a continuation of the business cycle research associated with Arthur Burns and W.C. Mitchell that was undertaken at the National Bureau of Economic Research. Koopmans (1947) savaged the Burns-Mitchell research methods as "measurement without theory".^x In reply, Friedman (1950, 489) noted (in defence of Mitchell, his mentor) that the failure to use high status modern language could destroy almost completely the influence of an economist. Friedman reflected that Mitchell's lack of a "more direct, obvious and far-reaching influence" (in a profession increasingly dominated by Walrasians and Keynesians) could be explained by his "own attitude towards his empirical work as expressed in his research program ... the elaborately casual language in which it is presented, and the extent to which its abstract elements are concealed ... He uses none of the jargon we have grown so fond of".

Did Friedman construct the natural-rate model so as to maximise its appeal to an audience that was captivated by high status Walrasian language? Or are those who estimate natural-rate models "Bastard Monetarists". It is unlikely that there will ever be a consensus about such motivational questions. Certainly, Friedman's (1968b, 5, n2) engagement with the economics profession was undertaken with strategic considerations in mind - against the "conditioned reflex[es]" of "entrenched Keynesianism". Equally, the natural-rate model appears to be a carefully constructed Chicago candidate, designed to challenge macroeconomic orthodoxy. There are similarities, even of language, between Walras' marginalist revolt against the widely-accepted labour theory of value, and Friedman's natural-rate revolt against the Keynesian hegemony: "any value in exchange, once established, partakes of the character of a natural phenomenon, natural in its origins, natural in its manifestations and natural in essence" (Walras 1954, 69). In constructing his counter-revolution, Friedman (1974a), a self confessed "collector of schools", was behaving *as if* he were a self-conscious revolutionary, aware of these historical precedents. In preparing his review of Walras, Friedman (1955, 907, n7) had access to a (then) unpublished doctoral dissertation on *The Rise of the Marginal Utility School, 1870-89*, by a Chicago student, Richard Howey. According to Howey (1989, xxiii, 38), Walras "had a plan for scientific revolution ... Later he sensed correctly that if he was to 'assume' measurability, the

less said about it the better".

Friedman (1953, 7) described theory as a "filing system", and the great contribution of the natural-rate concept is in providing a "filing system" for *all* conceivable inflation-unemployment observations, even if the natural-rate model remains unfalsifiable and untestable. Indeed, many macroeconomists *assume* measurability; or rather assume that estimates of the natural rate (however derived) exert some gravitational pull on the actual rate.

Monetary targeting may have petered out - but the Monetarist natural-rate model remains at the core of applied macroeconomics. This was, in part, because a Marshallian had placed a Walrasian concept at the core of an increasingly Walrasian discipline; despite his belief that Walrasian analysis has "value for a very different purpose. It is an extremely useful abstract conception to bring out the logic of the interrelation of the price system; [but] it cannot be used to analyse a concrete problem" (Friedman 1976, 26).^{xi} No doubt, fancier econometric footwork will continue to produce estimates of the natural-rate of unemployment. But these estimates will serve to mislead policy makers until the "concrete problem" of the gravitational pull on the actual rate is successfully addressed.

NOTES

i. Stigler (1962d, 71) thought the title invited a "slovenly stereotype"; it was also geographically inaccurate in that Friedman, he rather provocatively claimed, was the leader of the "Berkeley-Cambridge axis".

ii. Certainly, Leijonhuvud (1965) made a favourable impression on Friedman (1974b, 16, n7).

iii. Rose Friedman felt she had been the victim of "sex discrimination" at Koopmans' hands when he had her removed from their joint office shortly after being appointed to the War Shipping Board: "It colored my opinion of Tjalling then and later when, for some years, he was a colleague of Milton's at the University of Chicago" (Friedman and Friedman 1998, 109-110).

iv. Chicago economists continued to despair of the theory of expectations: "the promised land to some economists and a mirage to others. The reviewer must admit that he leans towards the latter view: much of the literature on expectations consists of obvious and uninformative generalisations of static analysis". With respect to "the revision of anticipations ... progress depends much more on the accumulation of data (of a type almost impossible to collect!) than on an increase in the versatility of our technical apparatus" (Stigler 1941, 358-9; see also Schultz 1949; Woking 1949; Boulding 1949; Norton 1949). Phillips solved the problem of the measurement of adaptive inflationary expectations for Friedman in 1952; Friedman was so impressed that twice, in 1955 and 1960, he attempted to recruit Phillips to the University of Chicago (Hammond 1996, 123, n15).

v. Schumpeter formed this judgement about the seminal importance of Walras' work in 1908, if not before (Hutchison 1953, 191-3). Also, in the 1930s, von Neumann and Wald published proofs of the existence of general equilibrium (Weintraub 1983; Debreu 1987).

vi. In an earlier attempt to provide a guide to "long-run objectives", Friedman (1953 [1948], 133, 144) stated that "Under existing circumstances, when many prices are moderately rigid, at least against declines, the monetary and fiscal framework described above cannot be expected to lead to reasonably full employment of resources ... The brute fact is that a rational economic program ... must have flexibility of prices (including wages) as one of its cornerstones".

vii. Paul Samuelson (1967a, 113) was a leading proponent of these three General Theory revolutions. In his essay in honour of Edward Chamberlin, Samuelson (just prior to Friedman's Presidential Address), stated that "a proper understanding of general equilibrium [is necessary] ... to attain ... an understanding of partial equilibrium".

viii. According Edmund Phelps (1996), the co-author of the natural-rate model, the natural-rate is a weak, not a strong, attractor; the system is perceived to be

path-dependent.

ix. Referring to the economic analysis of political institutions, Stigler commented that had Friedman "been what he likes to call a Walrasian instead of a Marshallian, the intellectual atmosphere would have been very inhospitable and uncordial to this kind of development". Stigler (1939, 471) also concluded that "The general equilibrium method is not fertile: we sacrifice content to formal generality until we achieve the state of the perfect dilettante, and know nothing about everything".

x. The disinflation component of the natural-rate model could be described as theory without adequate measurement.

xi. Friedman (1976, 25-6) was discussing demand curves: the Walrasian demand curve was derived by "mathematical economists" who were "unwilling to put anything" into the "Everything else in the world" category.